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Atmospheric Chemistry and Physics Discussions

Interactive comment on "ExchanGE processes in mountainous Regions (EGER) – overview of design, methods, and first results" *by* T. Foken et al.

Anonymous Referee #2

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The paper presents an overview of the EGER campaign, a measurement activity conducted at a forest site in south-eastern Germany focusing on the exchange of energy, matter and reactive compounds during two intensive field campaigns. Besides providing an overview of the overall goals, practical issues on the applied instruments, methodology, etc., there is a more fundamental research component on the issue of coupling regimes, the role of turbulent exchange inside/between the canopy and the overlying atmosphere in atmosphere-biosphere exchange of reactive nitrogen. Such papers are needed to provide the background information to other more focused publications on the results of the campaigns but the added analysis on the coupling regime also provides a more in-depth scientific analysis. Overall, I recognize that writing such

papers is quite challenging and do appreciate this effort and the contents such that I think it should be published in ACP after consideration of the number of more specific comments listed below.

The main focus seems to be on N exchanges whereas lately we see also more studies that address the issue on reactive carbon and also peroxide exchanges. It would be good to at least stress the main focus on N exchanges and possibly shortly including some references to this ongoing activities on the canopy interactions/dry deposition of reactive C species and peroxides.

Overall, the introduction doesn't make a very compelling point about the particular goals and added value of this campaign. Putting the explicit goals in bullets to discern more specifically from the information with all the background information would solve this problem.

The paper is very long which is what obviously happens describing such an extensive campaign including this large number of measurements. However, there are possibilities to shorten the paper; for example section 2.4.1 is very theoretical describing a commonly known issue on atmosphere-biosphere exchanges and which should be substantially shortened or even removed. Also the introduction of the Damkohler number addresses a basic feature of reactive exchange processes that should be commonly known to the interested reader and consequently, this section can also be substantially shortened. What appears to be main point to be taken from these two sections is on selection of reactive trace compounds, considered in the presented analysis, where it is anticipated that chemistry-turbulence interactions occur.

Introduction; might be useful explain in a little more detail what is exactly meant with coherent structures since this one of the main topics of this paper.

Lines 82/89; better understanding instead of deeper understanding

Line 246: the best logistical conditions; you really don't need this statement and if you

indicate “best” then you need to indicate relative to other sites.

Line 576; there should be an introduction of why this Kelvin-Helmholtz instability is introduced. This term now comes out of the blue and it would be useful to indicate the motivation to link the mixing layer scale to this Kelvin-Helmholtz instability. Actually this explanation is partly found in line 585 and so moving forward this sentence would already do the job.

The section on the models actually raises the issue on how these models have been applied to support the analysis. It now simply appears as if these models have been applied simply because of their availability. It is a comment similar to the one made on the introduction; there should be an explicit explanation why these models have been selected and how they have been applied in the presented study.

Section 3.1; it is completely clear from this section what the point is being made. Do you want to determine the roughness layer height or? It appears from the text that main information to be inferred is the correction function/enhancement factor for momentum exchange which was also discussed in section 2.5. However, there the parameter was only described and not defined yet. Doing so would be the link much more obvious.

Section 3.2; It is indicated that, referring to section 2.4.3, one can infer the contribution by the Reynolds averaged and “coherent” flux into the total flux. This is supposed to be illustrated by the top panel of Figure 9 but it is not clear what is actually shown there. Some more text on what is actually plotted should be added (not only in the Figure caption). And reading that you can actually distinguish the contribution to the coherent structures by ejections and sweeps I wonder how this is done. There is a reference in 2.4.3 to some of this part of the analysis but it would be very useful to at least provide a very short indication how this is actually done also since this a key part of the analysis.

Line 911: there seems to be a part of the text missing here (F-NH4+ and F-NO3)

Line 933; what are “dispersion of global radiation data”??

Line 963; I am surprised to read that the explanation for a night time zero NO flux is stomatal closure and negligible surface deposition. You would still have the large soil NO emission flux and which implies that for a zero flux you would a sink compensating for this source. It is most likely the O3 titration that does the job.

Section 3.5; I am aware that the conclusions are presented at the end of the ms but having this lengthy paper it would be very useful to see at the end of the sections, like this one, already a short conclusion from the particular section.

Section 3.6.1; add that the concentrations are calculated as C24m-C1m indicating that a positive value denotes a downward gradient and vice versa.

Line 1074; “is therefore not expected”

Section 3.7; Now that I have read this section I again raise this issue about the added value of the models. It should be clearly indicated in the beginning that the models have been applied to evaluate their representation of the coupling states through a direct comparison of observed and simulated latent heat fluxes. I initially also thought/anticipated that you were going to use the models to also evaluate trace gas fluxes, etc.

What does the term Re s[%] express in equation 11? Line 1154; what are aerodynamic submodules (have an idea but this wording is not clearly expressing what you are referring to) The information on how the models have been constrained is missing; were they driven by the observed micrometeorology? What about the assumptions on soil moisture conditions, etc. Line 1268; remove subscript fonts

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